XXV.—*On the Origin of some of the Lower Forms of Vegetation.* By Mr. Henry Oxley Stephens.

To the Editors of the Annals and Magazine of Natural History.

Gentlemen,

No one ever directed his attention to the œconomy of the lower forms of vegetation without soon arriving at the highly interesting but perplexing problem of their origin and reproduction. No question in vegetable physiology is of higher moment than this, none surrounded by greater difficulties, and in none is the inquirer more prone to error than in attempting conclusions from the negative facts (if the term is admissible) with which he has to deal. The obscurity in which, from its very nature, the subject is involved, is so dense, that many physiologists avoid it altogether as hopeless, considering it to be beyond human intelligence; whilst, on the other hand, some rash speculators, drawing inferences which the doubtful premises cannot warrant, descend at once into the profound of materialism *, and do not hesitate to intrude with unholy footsteps within the sacred precincts of forbidden ground. Nevertheless, the origin of the lower tribes of Fungi (for it is to these alone this paper refers) is a question as open to discussion, and as fit for investigation, as any other point of Natural History. There is no perfect Fungus which is not furnished in some part with an apparatus which bears certain minute bodies called sporidia, having some degree of resemblance to the reproductive bodies (sporules) of Ferns, Mosses and Hepaticæ, which last are well known to produce their like kinds by a process analogous to the germination of seeds. It has been assumed (and indeed generally admitted, though I am not aware directly proved) that these sporidia are the seeds of Fungi, producing by cryptogamic germination the same species as the parent plant; whilst other physiologists, admitting the sporidia to be capable of continuing the species, do not consider this to be the only method, or as indeed at all adequate to account for the production of Fungi in certain situations.

This is the question we are about to discuss. It is argued in behalf of propagation by spores, that these bodies, which are produced in such numbers as to be beyond all estimate, must have a definite office to perform, and that from their peculiar lightness, they are, as soon as shed from the hymenian of the parent plant, wafted through the air, and thus

* [It does not seem clear in what sense our Correspondent employs this term.—Ed.]

distributed to almost any distance. Whilst I admit the spores to be the reproductive bodies, I dissent from the latter conclusion, which seems very doubtful, as far as observation will carry us in this difficult subject. I am inclined to think the spores of Agarics at least are not generally disseminated far from the spot on which the plant which produced them grew.

I have always found Agarics which bear spores of a colour easily seen, the *Leucosporidia* for instance, shed them underneath the pileus; and that the grass, &c. is covered with the spores only immediately around and beneath the plant, to which they adhere, and are not blown away as soon as shed. It will be said this must necessarily depend upon the atmosphere, whether the air is still or not; but the generality of Agarics lie very close to the ground, and the expansion of the pileus, extended like an umbrella, must, I think, even in windy weather, prevent any great current of air from passing under it. Perhaps this will be thought over-refined reasoning, but the following facts seem to countenance it. Particular species appear annually on the same spot of ground, and do not wander away from it; thus *Ag. nebularis,* Batsch, grows every autumn at one corner of Leigh Down, and does not spread from this locality. *Ag. personatus,* Fries, I have seen every autumn in the same situation for several years. *Ag. oreades* grows in eccentric circles, one circle exteriorly to that of last season, which would scarcely be the case if the sporules of this species were scattered about by the wind; but single individuals do occasionally occur: no doubt *Ag. oreades* is propagated in these circles by underground mycelia, but this circumstance does not affect the argument. The stump of a tree immediately under a plant of *Polyporus igniarius* will be seen densely dusted with sporules, showing that these fall near the spot occupied by the parent plant. It would be easy to multiply instances of the regularity with which Fungi appear in the same spot, but these are enough; and from them I infer that the dissemination of the spores of Fungi through the medium of the atmosphere has been greatly over-estimated. Taking into account the number of species of Fungi, and the great quantity of sporidia which each plant gives out, it seems to me the atmosphere must contain so many, that they would certainly be detected in it before this time, and yet none have ever been observed. The method of making what is called mushroom-spawn is too well known to need description, and every exhausted hot-bed will produce plants of *Ag. campestris*: how did the germs of these plants get there? It is answered, through the air: this has been considered before; besides, the mycelia will be found in the com-

post, several feet below the surface. But the spores were introduced with the horse-dung; true, but this is produced by stabled horses fed on hay, and the *Ag. campestris* does not grow in mowing grass, nor in the hay-making season, and the hay could scarcely contain such abundance of spores; besides, is it probable such minute and delicate bodies would pass through the digestive process of the animals' intestines entirely unaltered? It may be answered, oats frequently do; but the two cases are not alike. There is every reason for believing Fungi are produced from spores: the circumstance of exotic species appearing in foreign mould, *e. g. Aseroe rubra*, La Billardière, in earth from New Holland, seems to prove this; but to conclude they are invariably so produced, appears to me to be assuming much more than we really know, and that in the face of circumstances which render the reverse very probable.

In support of the hypothesis of the invariable origin of Fungi from spores, it is stated that wheat selected from specimens infected with *Uredo caries*, when sown, produces a bunty crop. This is scarcely a satisfactory argument, for such wheat may possess the tendency to this disease without being actually impregnated with the spores, just as we know the finest samples of wheat from hot climates produce mildewy crops when sown in our more cold and changeable latitude; besides, the experiments of Sir J. Banks with wheat sprung from sources infected with *Puccinia graminis* led to an opposite conclusion. It may be advanced, that wheat growing near Barberry bushes is rendered mildewy by infection from the parasitic Fungus frequently abounding on those trees; but the Barberry parasite is an *Æcidium*, whilst the plagues of wheat are always *Pucciniæ* or *Uredines*. The greatest difficulty is in the erumpent Fungi; how could they reach the situations in which they vegetate? It may be said precisely the same question arises in Zoology, and the arguments which apply to *Entophytes* will apply to *Entozoa*; this is only advancing a counter-difficulty, and the solution of one problem would probably explain the other. *Entophytes* must have their origin in one of the following methods: their spores must be introduced into the parent plant, and there, finding a suitable nidus and circumstances favourable for their growth, commence their proper vegetation; or the structures in which they grow must, under favourable conditions, have the power or quality of originating them out of their own organization.

There are difficulties in the way of both these theories. Of the first, the question arises, how are they introduced into the parent plant? There appears to be only two ways,

either by the spongioles of the roots, or through the stomates of the leaves. As far as our knowledge of the physiology of the spongioles extends, it seems improbable that they have the power of absorbing solids (for earths, etc., as silica, are absorbed in a state of solution), even when as minute as sporidia; nor does our imperfect acquaintance with the course of the sap enable us to say, that along with it innumerable spores can pass the long journey from the roots of an oak or pine tree to the leaves; this would be altogether an unproved assumption.

Besides, if these sporidia passed through the vascular tissue of plants, of late years these structures have been examined with such scrupulous and scrutinizing care through the most powerful microscopes, I should have conceived a stray spore must have been observed before this time, and yet none have ever been seen, simply, I presume, because there are none there to see. But they may be absorbed through the stomates into the leaves, and carried down along with the woody fibres, which, according to the theory of Du Petit Thouars, the leaves annually form. It is first necessary to prove the openings of the stomates are large enough to allow the sporidia to pass through them. If a sporidium of *Uredo* or *Puccinia* is a single plant reduced to the most simple condition, and not admitting of further subdivision, it is too large to find access to the external structure of the leaf through the orifices of the stomates; but it may be said the grumous contents of the sporidia of *Puccinia* and *Uredines* are prolific, and can pass through the stomata. In our present state of ignorance on this obscure subject, we can neither prove nor disprove this point, and I would wish to state every argument as fairly as I am able. Evergreens, which have thick coriaceous leaves and a horny cuticle, are well known to be very scantily supplied with stomates, and yet such leaves are prolific of entophytous Fungi, *e. g.* the leaves of Lauro-cerasus, holly and ivy; this is a negative argument against the introduction of spores through the exhalent orifices.

It is easy to produce some erumpent Fungi; you have only to break a living twig of oak so as to cause it to wither and die, leaving it attached to the tree, and *Cenangium quercinum* will appear beneath the bark. Now if this Fungus arises from spores already contained in the tree, and only waiting for the death of the twig to assume an active state of vegetation, the whole of the branches of the tree must be impregnated with countless multitudes of the spores of *Cenangium* (not to say of many other Fungi), which is an assumption

for which we have not the slightest shadow of evidence. There is scarcely a stick that died in the autumn, which is not, on the approach of winter, densely covered with *Tubercularia vulgaris*; there is no proof, when the stick was living, that the spores of this plant remained dormant within its cellular tissue.

It must be recollected that, admitting the hypothesis of the absorption of spores, the earth or air must be impregnated with them in countless myriads, and these most delicate globes or cells must possess a most extraordinary power of resisting putrefaction; and in the case of *Cenangium* preserve their vitality through an incredible space of time when imprisoned in the solid structure of the oak-tree; and yet the origin of Fungi, which are parasitic upon Fungi, according to the theory of their invariable production from spores, is still more inexplicable. The elegant little *Ag. Loveanus*, Berk., has its origin within the substance of the pileus of *Ag. nebularis*, Batsch, and bursts through the cuticle of the pileus of the latter plant. Now the parent plant is altogether cellular, having neither vessels or tubes of any kind through which we can suppose the spore, which gave origin to the *Ag. Loveanus*, could be transmitted.

I do not know how this can be explained according to the theory of absorption of spores contained in the earth.

The other theory is, that the structures which contain erumpent Fungi must, under certain favourable circumstances, have the power or quality of originating these plants out of their own organization. Inclined as I am to suppose some of the lower forms of vegetation may obtain their existence out of the ruins of the higher, according to certain definite laws imposed by the great Author of all things, which laws are to us altogether unknown, I should be sorry to be thought to be an advocate of the doctrine of what is called spontaneous generation; in plain language, things making themselves; it is too absurd to need disproof; or of equivocal generation, for nature emerged too perfect from the hands of her Creator to have anything doubtful or equivocal in any of her processes. These processes may *seem* doubtful or equivocal to us, simply because they are beyond our comprehension. Of the truth of the last theory of production of imperfect plants, it must be admitted there is no direct proof; it must rather be inferred from the difficulties and objections which have been advanced against the former. It is generally assumed by those who accept the latter theory, that out of the departing vitality of some higher organized vegetable (for I have considered the question throughout as referring to

vegetable life only) a lower degree of life and organization may arise, like the fabled Phœnix, from ashes, and thus the simpler forms of vegetation may derive their origin from the upper; but it must be admitted there are objections to this assumption, and those not of theory and speculation, but of fact and experience. Many erumpent Fungi have not their origin in dying vegetable matter, but in substances which have long lost all vitality, and therefore can part with none to the parasites which infest them;—*Sphæria entypa*, for example, which grows within the substance of wrought wood, such as posts and rails, the origin of which cannot be explained satisfactorily according to the latter theory of the production of imperfect vegetables. The whole subject is as interesting as obscure; and it is possible that an observer who had time and leisure for tracing, with the assistance of a microscope of sufficient power, the growth of some Fungus of the lowest organization, such as *Tubercularia*, might arrive at the ultimate point of its origin, and be enabled to decide whether it had its being from a metamorphosis of the organized structure of the parent plant, or sprung from a spore, and derived its nutriment only from the material in which the germ of the parasite was previously deposited.

Henry Oxley Stephens.

Terrell Street, Bristol, March 12, 1841.

XXVI.—*An Amended List of the Species of the Genus* Ovis.

By Edward Blyth, Esq.*

The arrival of various spoils of different species of wild sheep, since my memoir upon this genus of animals was read before the Society, enables me now to clear up several points which I formerly left as doubtful, as well as to include some additional species in the catalogue, and to indicate still more as probably distinct, and therefore desiderata to which the attention of travellers and others should be directed.

1. *Ovis Polii*, nobis (the Pamîr Sheep). In the narrative of the celebrated Venetian traveller, Marco Polo, we read (in Marsden's edition, p. 142) that upon the elevated plain of Pamîr, eastward of Bokhara, and which is 16,000 feet above the sea-level, "wild animals are met with in great numbers, particularly sheep of a large size, having horns three, four, and even six palms in length. The shepherds form ladles and vessels of them for holding their victuals.

* Read before the Zoological Society, July 28, 1840. The notes, bringing the subject up to the present state of information, are now added by the author for publication in this work.